Biogeosciences Discuss., 7, C2401–C2404, 2010
www.biogeosciences-discuss.net/7/C2401/2010/

Biogeosciences Discussions

Interactive comment on "Satellite observations reveal high variability and a decreasing trend in CO_2 fluxes on the Scotian Shelf" *by* E. H. Shadwick et al.

Anonymous Referee #2

Received and published: 20 August 2010

This paper presents pCO2 and chlorophyll a concentration data from an autonomous mooring on the Scotian Shelf for the 15 months between April 2007 and June 2008. These data were used, along with satellite data and other measurements, to derive an algorithm which was then used to calculate pCO2 over wider spatial and temporal scales. The authors use the results of the extrapolation to hindcast surface ocean carbon dynamics for the decade 1999 to 2008. A similar approach has been used by other researchers in several other oceanic areas, and provides a useful method for estimating CO2 variability on a larger scale than is possible using conventional measurement techniques.

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Interactive Discussion

Discussion Paper

The study is worthy of publication, however there are some issues that need to be addressed first.

The paper is, on the whole, well-written, although there are several spelling and grammatical errors (word repetition), and cross-referencing errors. The paper would benefit from a rigorous proof-read.

Specific comments

A paragraph should be included briefly outlining the physical, biological and chemical oceanography of the study area, and Figure 1 should be modified to include the geographical features mentioned in the text. The location of the Sable Island Meteorological Station where the atmospheric CO2 and wind speed were measured should also be indicated on Figure 1, and the distance from the CARIOCA deployment site to Sable Island should be mentioned in the text.

Local time should be included as well as UTC (page 5272 line 11).

Monthly integrals of the gas transfer velocity were determined from hourly wind speed data. A measure of the variation in the wind speed and the resulting gas transfer velocity occurring in this integration process should be given.

Equation (1) is a key component of this work. More information needs to be given on the contribution of each term to the equation, the error associated with each coefficient and the improvement of the fit when the additional terms are included. This information could be included as an extension to Table 2. Why is k included in Eqn (1) and not wind speed?

It appears that the monthly mean CARIOCA pCO2 data were used in the derivation of equation (1) (page 5272 line 25), then values computed using equation (1) were compared with pCO2 measured by the CARIOCA buoy (page 5274 line 23), this is a circular argument. It should be made clear which data is used in the derivation of equation (1), and which data is used as verification. Obviously these should not be

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the same data set. The use of shipboard measurements and values calculated from alkalinity and DIC for verification of Eqn (1), as was also done, is independent.

The SST and ïĄĎpCO2 anomalies have been defined (Page 5274 line 21) however these values are not referred to again in the manuscript. The anomalies should be presented and commented on, as this is the measure of how good Eqn (1) is at predicting pCO2.

The wind speed was measured only at the Sable Island site, therefore the same value of k was used in each grid box in the hindcast? Some comment on the wind speed variability should be made, particularly any land / coast effects.

The CARIOCA buoy was moored close to the coast, and the ChlF and ChlSat was "roughly" 1:1 (page 5274 line 15), however explicit mention should be made of the shortcomings of the SeaWIFS data in coastal regions due to CDOM and suspended particulate matter, and comment made on the impact this may have on the work presented here.

Which equilibrium constants were use in the calculation of pCO2 from DIC and TA (page 5275 line 7)?

CO2 flux from the ocean into the atmosphere is defined on Page 5276 line 12 and in the table 3 caption as negative, however ïĄĎpCO2 is defined as pCO2(ocean) – pCO2(atm) on Page 5282 line 18. ïĄĎpCO2 defined in this way would give a positive flux when the ocean is a source of atmospheric CO2. The usual convention (eg Takahashi et al DSR II 2009) is that ïĄĎpCO2 = pCO2(ocean) – pCO2(atm) and flux from the ocean to the atmosphere is positive.

What does "... are compared with the same high precision, ..." (Page 5276 line14) mean, and what statistical evidence do you have for this statement?

The sentence ".. The ChlF coefficient corresponds..." (Page 5277 line 1) refers to a range of 115 mgC (mg chla)-1. 115 is not a range.

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What method was used to estimate the mixed layer depth (Page 5277 line 15 and Fig. 5)?

Page 5278 line 5 refers to equations (5) to (7) not (4) to (6), Page 5278 line 6, refers to equation (5) not equation (3).

The cross-spectrum method used to examine the relationship between pCO2 and SST should be referenced. I am not familiar with this technique, however it seems to be over dominant in this paper. I suggest that this section be shortened.

The authors infer that the NAO could be influencing the CO2 dynamics in the Scotian Shelf area, however the reasoning is vague. The NAO link is mentioned in the abstract, however the justification in the text does not warrant this. The section should be expanded.

More context on the East China Sea should be given, otherwise the comparison with the Scotian Shelf is irrelevant. The sentence introducing the East China Sea comparison (Page 5284 line 3) does not contain a verb, therefore makes no sense.

The lack of agreement between the observations and the modelling studies (Page 5285 line 2) needs further comment.

Fig. 6 shows the change in pCO2 for each of the contributing factors , how was the initial pCO2 determined, and why isn't it the same for each contribution? Similarly, for Fig. 8)

Figure 9 is not necessary. If the authors decide to retain Fig. 9 then the caption should be expanded to explain the meaning of Wind + and Wind -.

Interactive comment on Biogeosciences Discuss., 7, 5269, 2010.